PROPOSED ZOOLOGICAL STATION AT GRANTON, NEAR EDINBURGH

AT the half-yearly meeting of the Scottish Meteorological Society held at Edinburgh on Thursday last week, Mr. John Murray, convener of the Society's Fisheries Committee, submitted the following Report:—

"The Fisheries Committee of the Council appointed in February last have had under their careful consideration the matters remitted to them by the Council, viz. the carrying out of investigations in accordance with the terms of the grant of 1500*l*. made to the Society by the Executive Committee of the International Fisheries Exhibition held in Edinburgh in 1882. The Committee recommends (1) to continue and extend the river observations and the observations made by the District Fishery officers through the Scottish Fishery Board, and to discuss all observations made to the end of the fishing season of 1883, which are yet undiscussed.

"2. To obtain the assistance of a few naturalists in making observations at several of the chief fishing centres and principal inland lakes. Prof. Herdman has consented to reside at Loch Fyne for a month, and to arrange for observations for a year. Mr. Hoyle is in like manner to go to Peterhead, and Mr. Beddard to Eyemouth. The Rev. Dr. Norman has during the present month been engaged in examining a large number of the Scottish lochs. Instructions have been drawn up for the guidance of these gentlemen, and a sum not exceeding 50*l*. has been placed at the disposal of each for the expenses immediately connected with the investigations. These observations are of a strictly tentative character, but will certainly lead to additions to knowledge, and are, moreover, necessary as a basis for further investigations.

"3. The Committee have had under consideration the recommendation of the Executive Committee of the Fisheries Exhibition as to the foundation of a zoological station. A number of the members of Committee have examined the capabilities of the old Granton Quarry, which has been for many years in direct communication with the sea, as a suitable position for a zoological station.

"The convener has drafted the following scheme, which in the opinion of the Committee would, if carried out, afford excellent facilities for biological researches and meteorological observations bearing upon these inquiries:—It is proposed to inclose the Granton quarry, which has an area at high water of about ten acres, and depths varying to sixty feet, so as to regulate the inflow and outflow of the tide in such a manner that while admitting abundance of sea water at each tide, fish and other animals will be prevented from escaping out of the inclosure. This will be done by means of stakes and wire with other kinds of netting. The quarry will then be stocked with all kinds of fish and marine invertebrates. When it is desired to separate fish or other animals for special study this will be done by floating or fixed wire and wood cages.

"A barge, about 64 feet by 27 feet, of great stability, will be moored in the inclosure; upon this will be built a house with laboratories, workrooms, and a library; it will also be furnished with a small windmill to pump up sea water into a tank on the roof. The water in this tank will be conveyed by pipes to the various tiled tables, glass jars, and aquaria of the establishment. A small cottage will be built on the shore for the accommodation of the keeper and engineer, with one or two spare rooms. A steam pinnace for dredging and making observations in the Firth of Forth and the North Sea will be attached to the station.

"A naturalist will be appointed whose duty will be to make continuous observations and experiments, assisted by the engineer and keeper. There will be ample accommodation for four other naturalists to work at the station and carry on investigations; and, so far as the accommodation will permit, British and foreign naturalists will be invited to make use of the station free of charge.

"Towards the carrying out of this scheme the Duke of Buccleuch has liberally granted a lease of the quarry at a nominal rent, with permission to erect a cottage on the shore; and Mr. Howkins, his Grace's local commissioner, has promised all the assistance in his power to further the undertaking. A gentleman who takes a warm interest in the progress of research in Scotland has offered 1000*l*. to construct the barge and fit it up with laboratories and workrooms. Mr. John Henderson (of Messrs. D. and W. Henderson, shipbuilders, Glasgow) has undertaken to provide the plans and specifications of the barge and laboratories gratuitously; Mr. J. Y. Buchanan has promised to fit up one of the rooms on the barge as a chemical laboratory suited to the requirements of the station; Mr. Thomas Stevenson, the Society's Honorary Secretary, has agreed to give his professional services in inclosing the quarry gratuitously; and Mr. John Anderson, of Denham Green, has undertaken to provide the station with a salmon and trout hatchery. The convener will furnish the laboratories with apparatus, and place his large zoological library at the service of workers. A number of gentlemen have promised to support the undertaking when once commenced; and the convener believes that within a few months he will be able to announce that the station has been presented with a steam pinnace and with funds for the erection of a cottage on the shore—the only desiderata to complete the scheme.

"In these circumstances the Committee, believing that this scheme deserves their hearty support, recommend, for the year ending November 1, 1884, a grant from the Fishery Fund not exceeding 300*l*., and 250*l*. for each of the two subsequent years, towards the expenses of the station, on the conditions that the biological and meteorological observations and the investigations above referred to, relative to the Scottish fisheries, be carried on, and that a report on the work done be annually furnished to the Council of the Society."

The above grants were agreed to, and it was announced that the works at Granton would be commenced at once. It is expected that by the beginning of November the proper work of the station will be begun. Already, we understand, several distinguished naturalists have signified their intention to avail themselves of the altogether unique facilities which will be afforded by this zoological station for the successful prosecution of biological research. It is gratifying to observe the heartiness with which the funds required for carrying out this admirable scheme are being provided, and it cannot be doubted that the 800*l*. still required for the steam pinnace, the 500*l*. for the cottage, and the 200*l*. for inclosing the quarry will also be soon provided by some of our more generous patrons of science.

ELEVATION AND SUBSIDENCE; OR, THE PERMANENCE OF OCEANS AND CONTINENTS

IT has been observed, and with increasing frequency within the last few years, that wherever considerable weight is added on any part of the earth's surface, a corresponding subsidence of its crust almost invariably follows. It is generally admitted that nearly the whole of the sedimentary rocks, enormous as their known thickness is, were deposited in shallow water, and therefore in slowly subsiding areas. The Palæozoic rocks consist mainly of sandy and muddy sediment, with occasional intercalated zones of limestone. They everywhere bear witness to comparatively shallow water and the proximity of land. Their frequent alternations of sandstone, shale, conglomerate, and other detrital materials, their abundant rippled and sun-cracked surfaces, marked often with burrows and trails of worms, as well as the prevalent character of their organic remains, show that they must have been deposited in areas of slow subsidence, bordering continental or insular masses of land.[1] Vast thicknesses of strata have been continuously deposited at or near the sea-level. The coal-measures present a series of alternating layers of vegetable matter and brackish water sediments, reaching in the South Wales district a thickness of upwards of 10,000 feet, whose accumulation must have been accompanied almost foot by foot by a corresponding subsidence. The Cambrian sediments accumulated in the British area to a thickness of 23,000 feet apparently without any great change in the depth of the sea in which they were formed; and throughout the deposition of the whole of the Silurians, subsidence seems to have kept pace with sedimentation. The Permian again furnishes many instances of sedimentation at or near the sea level sustained throughout great thicknesses, and of frequent alternation of marine and freshwater deposits. Among Mesozoic rocks, the New Red Sandstone furnishes an example of isolated basins of deposit to which the sea found repeated access, though a thickness in places of 3000 feet had accumulated in them. The German Triassic basin is for the greater part of its thickness a succession of terrestrial deposits containing plant-remains. The Jurassic and Cretaceous systems were deposited during interrupted depression of the sea bottom, while the Tertiaries abound with local instances in which subsidence has kept pace with sedimentation.

The washing of sea-coasts and removal of material shown by the discoloration of the sea for miles round the shore in stormy weather, shows that the process of accumulation of sediment still progresses on a very large scale.[2] It has been ascertained that nearly the whole of this must be redeposited within a distance of thirty miles. If the waves have no disturbing power at a greater depth than 40 feet, and could therefore neither deepen the sea-bed nor prevent its silting up to within that depth, our shores should be surrounded by enormous tracts of shoal water, whose bottom might be grooved or deepened by local currents, but whose average depth would not exceed 40 feet, or even less, since many rocks are so protected by seaweed that their further degradation when once below the reach of surf must be inappreciable. There is no cause therefore capable of generally deepening the sea round coasts beyond some such limit as this, except subsidence, and this can only be ascribed with any semblance of probability to the accumulating weight of sediment. The prevailing tendency on sea-margins is and must be towards depression, and there are few residents on the sea-coast who would be unable to contribute valuable observations on this point. It must however be remembered that while raised beaches are conspicuous objects, depressed beaches could obviously hardly ever attract attention, even if the shingle had not been removed by the surf, and further, most, if not all, of the existing raised beaches may have been formed during the general elevation of the land that took place at the close of the glacial period.[3] Other observers have recorded numerous submerged forests on the coasts of Cornwall, Devon, Somersetshire, and Wales. An elevation of the coast may, on the other hand, be sometimes accounted for when due consideration is given to the surrounding conditions.[1] It may, for instance, conceivably be produced on any shores where considerable sediments are forming at some distance out to sea or where masses of cliff are being washed away.

The extreme sensitiveness of the earth's crust to any changes in the distribution of weight on its surface is, however, best exemplified by those local depositions and removals of matter which have attracted more general attention at the present day. The chief of these is the transfer of matter by river action from large tracts, and its accumulation in such limited areas as plains, estuaries, and deltas. Borings of 400 and 500 feet have shown that these often consist of long successions of silts, which alternating layers of shells and of vegetable matter prove to have been deposited at or near the sea-level, and the Wealden and Eocene formations in the British area show that such accumulations may exceed 1000 feet in thickness. In the case of deltas, subsidence must keep pace almost foot by foot with the accumulation, and be confined to the area over which the sediment is being deposited, for any more rapid subsidence would check its growth and convert it into an estuary. This sinking is apparently of universal occurrence.

A similar instance of the transfer of weight from larger areas and its precipitation on a very circumscribed area[2] is seen in coral atolls and reefs. The explanation of their formation given by Darwin requires a gradual subsidence keeping pace with their growth, which takes place within twenty fathoms of the surface only. This theory, simple and admirable as it is, accounting satisfactorily for all the observed phenomena of coral growth, has been contested by Mr. Murray, who has shown that atolls might be merely incrustations of volcanic peaks. But his theory seems improbable by contrast, for it demands 290 volcanic peaks at the sea-level in the Pacific coral area alone, every foot of which has been completely concealed by coral growth, though few volcanic craters are known so near the sea-level outside this area. We seem thus to have in coral growths another evidence of subsidence keeping pace with the increase of weight, sometimes, as soundings prove, to a depth of 1000 feet or more. The replacement of a column of sea water 100 fathoms in depth, by a column of limestone, would increase the pressure per square fathom from 619½ tons to 1487 tons, so that it is easy to realise how vast must be the increased pressure on such an area as that occupied by the great reef of Australia, 1250 miles long and 10 to 90 miles broad.

The sands, gravels, and clays, with marine shells and erratic boulders, prove that a great submergence took place during the Glacial Period, while Europe was under an ice sheet 6000 feet thick in Norway, and diminishing to 1500 in Central Germany. The extent of the submergence has been perhaps understated at 600 feet in Scandinavia, and was at least 1350 feet in Wales. A corresponding re-elevation accompanied the disappearance of the ice. It has often been supposed that the sinking of the west coast of Greenland is similarly due to its ice-cap.

It is probable that great outflows of lava may in like manner occasion subsidence—though it is by no means so well ascertained a fact. I have, however, observed in Iceland that lava-streams very frequently terminate in or flow under lakes or gulfs of the sea, though water presents no obstacle to their continued progress. Lakes have been filled in solid by outpourings of lava, and had those I observed existed previous to the flows the lava must have entered them in a more abrupt manner, and it seems therefore likely that they are depressions caused by the weight of the lava. But there are also instances in which the actual depression produced by the weight of lava-streams can be seen. A great lava flow has at some period debouched from Skjaldbreith, and from two other nameless craters to the south-east, on to the historic plain of Thingvállir, forty square miles of which is water. At its northern end the lava is still in its original position upon the slopes, but the whole central mass in the plain has torn itself away from the sides and sunk a hundred feet, leaving vertical cliffs of solid lava of that height on its flanks.

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[1] "Text Book of Geology," Geikie, 1882, p. 647.

[2] M. Marchal has estimated that the sea deposits annually 600,000 cubic metres of sediments in the Bay of Mont St. Michel, and 10,000,000 on the coasts of Flanders, Zealand, and Norfolk.

[3] My own experience on the south coast is that the weight of evidence points to a general sinking, for vestiges of submerged land vegetation and traditions of submergence are very frequent. At Bournemouth I have seen heath plants and roots, fresh-looking except for the incipient formation of pyrites, cast up from a tract of moorland now below the sea-level. Poole Harbour would long since have been left by the sea if there were no subsidence, and a landing-stage with rings found below low-water mark furnish valuable data as to the amount that has taken place in historic times. The Solent must have been originally a harbour like that of Poole, continually silting, sinking, and enlarging, the depression travelling west and cutting one river after another from the sea until the western channel was at last opened. The immense accumulations of mud in its channel seem to have dragged the land into a sort of trough with raised sides, so that the Yar, Medina, and Brading Rivers flow inland instead of out to sea. On its margins we find there have been oscillations of level, caused perhaps by alterations in the position of the sediments through changing currents. The inroads of the sea at Pagham and Selsea show the downward movement to have extended along the whole of the Hampshire coast.

[1] An elevation, for instance, has taken place on the Kentish coast which has closed the Stour to navigation and caused the sea to retreat from Stourmouth, Richborough, and Sandwich, and which is also marked by the great exposures of Eocene along this part of the coast above low-water mark, and which could hardly exist where exposed to strong tidal and wave action, unless the abrading process were counteracted. The immense deposits, taking place at a distance from shore, brought down by the Thames, must lead to considerable subsidence in its estuary and consequently some corresponding elevation along its shores. The Thames sediment is of unknown depth, but on its margins at Sheerness the alluvial mud is 80 feet thick, and at Upchurch, opposite Queenborough, 75 feet.

[2] But 3½ per cent. of solids preexisted in the water displaced by the rock.

Again, near Mývatn there is an immense tract of lava, the latest contribution to which, estimated by Mr. Lock at 31,000,000,000 cubic feet, welled out in 1875. This tract, known as the Örœfi, presents a somewhat analogous instance, for the centre of the flow has also broken away from its flanks and sunk. Mývatn, a lake of some thirty miles in extent appears to have been formed by the weight of lavas which have poured on to the plain from nearly every side. Another recent stream in the same neighbourhood, whose source and age are unknown, follows the course of the Skjálfandafljót to the sea and terminates in a deep gulf. It appears that, as long as lava-flows occur in mountainous regions or in narrow valleys, any subsidence occasioned by the additional weight is difficult to detect, but as soon as it enters on to plains the subsidence is marked. It may also be that valleys in undulated or folded strata, being inverted arches, would resist pressure, while even in valleys of erosion much of the pressure must be exerted obliquely against the mountain masses instead of wholly vertically as on a flat surface. It has been suggested that the discharge of masses of lava at the surface would leave cavities in the interior and thus occasion subsidence, and it has even been anticipated that Iceland would bodily disappear from this cause, like the island of Friesland, from the maps; but there is no evidence of any such cavities having existed in old basaltic formations or in volcanic districts, and it is far more probable that the escaped matter is pressed out by other lava which immediately replaces it.

Dr. Fisher believes that all plains in proximity to mountain chains, upon which the material provided by their denudation is spread out, sink under the weight of the material and cause a compensating elevation of the neighbouring mountains. The sub-Himalayan range consists of subaërial deposits from 12,000 to 15,000 feet thick brought down by torrents, and which must have been deposited on a level and continuously sinking plain. "The conclusion seems irresistible that corresponding to the long, though occasionally interrupted, depression of these plains, a correlative elevation of the great range which has supplied the deposits has been going on."[1] If, as in the Himalayas, the region be one of approximate equilibrium, and much sediment is brought off the mountains and spread over the plains, the mountains become after a while too light and the plains too heavy,[2] and accordingly the mountains rise and the plains sink to restore the contour. This appears to be what has happened.

These comprise nearly all observable instances in which weight has been transported from elsewhere to areas where it did not previously exist, and are sufficient to prove that in such cases a subsidence more or less equalling in amount the vertical thickness of such added matter—except in the case of ice, which is of a much lower specific gravity—nearly invariably follows. Can it be reasonably maintained that these subsidences and the reelevations, which seem invariably to accompany the removal of weight, whether by melting of ice, as in the glacial period, or by denudation, are not the result of the increase or diminution in pressure? If the accumulation of sediment were due to the subsidence, instead of the subsidence to the accumulated sediment, as recently suggested by Dr. Geikie, it would be most improbable that they would so frequently bear such near proportion the one to the other. In none of these instances has the subsidence exceeded the accumulation, as must sometimes have been the case if the sediment merely accumulated because a subsidence quite independent of it happened to be in progress.

Such subsidences would only be possible with a substratum somewhere of viscous matter. Professors Shaler and Le Conte and Mr. Fisher, and many other very able geologists, have advocated the existence of a fluid or viscous layer between a solid interior of great density and a consolidated crust. If, Mr. Fisher maintains, it requires great pressure to solidify the materials at the temperature of the solid interior, a melting temperature may exist at some depth before the pressure is sufficient to solidify. Although Prof. Geikie and many other geologists do not admit the continuous existence of such a layer, it is difficult to see how they escape the conclusion. In his text-book[1] Prof. Geikie states that "from the rate of increment of temperature downwards it is obvious that at no great depth the rocks must be at the temperature of boiling water, and that further down, but still at a distance which relatively to the earth's radius is small, they must reach and exceed the temperatures at which they would fuse at the surface." Further on he explains that the crystalline rocks of the Highlands of Scotland and of the Green Mountains of New England are mechanical sediments metamorphosed chiefly where they are most highly contorted, or have been subjected to the greatest pressure. Strata of sedimentary origin which have accumulated to thousands of feet in thickness may be depressed deep beneath the surface and brought within the influence of metamorphosis,[2] and be eventually reduced to a soft and pasty condition, and protruded into some of the overlying less metamorphosed masses in the form of granite veins, or be erupted to the surface in the form of lava. This is an absolute admission that at some depth, relatively not great, pressure converts solid into viscous or fluid strata. He further states that "There can be no doubt that the lines of equal internal temperature (isogeothermal lines) for a considerable depth downward, follow approximately the contours of the surface, curving up and down as the surface rises into mountains, or sinks into plains;"[3] so that it seems difficult to understand why the particular line of temperature or of pressure at which most rocks melt, should not be continuous.[4] Like conditions must produce like results, and if the mere pressure of overlying strata can anywhere or at any depth render rocks molten or fluid, they will become molten or fluid wherever the required pressure occurs. A nucleus kept solid at a temperature higher than its melting-point, through excess of pressure, cannot pass into a crust whose solidity is due to lowness of temperature, through absence of pressure, without the existence of that intermediate stage of pressure or temperature requisite to produce a melted zone or layer. Prof. Geikie in fact himself admits "that the nucleus though practically solid, is at such a temperature and pressure that any diminution of the pressure by corrugation of the crust or otherwise, will cause the subjacent portion of the nucleus to melt."[5] But as the pressure diminishes gradually throughout the crust from the enormous amount on the solid nucleus to the merely atmospheric pressure at the surface, how can the conclusion be avoided that there is everywhere a point in the earth's crust at which it must be just sufficient to keep rocks at the melting point. It seems utterly impossible, if it is once conceded that pressure does render rocks fluid, to avoid the conclusion that there everywhere exists a viscous substratum following to some extent the contour of the earth's surface.

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[1] "Physics of the Earth's Crust," p. 81.

[2] *Ib.*, p. 83.

[1] *L.c.*, p. 289.

[2] Geikie, "Text-Book of Geology," p. 587.

[3] *L.c.*, p. 287.

[4] Fisher maintains that mountain chains have solid roots, far exceeding their bulk above ground, projecting into the liquid layer.

[5] *L.c.*, p. 265.

Such a condition is precisely that which will alone explain the undoubted fact that the addition or removal of even comparatively small weights produces corresponding changes in the previous level of the earth's surface. "The deposition of 1000 feet of rock will, of course, cause a corresponding rise in the isogeotherms,"[1] that is, of the liquid layer, and "denudation of the land must lead to a depression of the isogeotherms, and a consequent cooling of the upper layers of the crust." Dr. Fisher[2] proves mathematically, in fact, that a liquid layer and no other condition can explain the movements that have taken place in the earth's crust, and satisfactorily account for volcanic action.

It appears in the present state of knowledge almost impossible to estimate the depth at which a viscous layer could exist. The estimates that have been made vary from 1000 miles to only 50,000 feet. On the one hand, however, if it is a fact, as Dr. Geikie surmises, that sedimentary formations of Silurian age have been fused and rendered viscous mainly by the mere superincumbent pressure of more recent sedimentary formations, the depth at which a viscous layer can exist must be less than the lowest estimate yet formed. On the other, the observed increase of temperature, not exceeding at most 1° F. for every 50 feet of depth, the melting temperature of rock, 2000° to 3000° F., would not be reached at a less depth than 100,000 feet. This is obviously too great a depth to account for some of the observed facts of geology, and is without any allowance for the increasing density of rocks at great depths, or for the many unknown agencies which may contribute at such depths to lower their melting temperature. The inert weight of 25,000 feet of rock of the density of slate, the thickness which according to Dr. Geikie has reduced rock to a viscous state, is about 2000 tons to the foot. I am not aware that any estimate has been made of the actual amount of heat that would be produced under such conditions. If, as must be the case, any relatively small increase of pressure produces a displacement in the molten layer, a compensating elevation must take place elsewhere, and if its effects are so considerable when the weights are relatively small, the results of pressure applied to oceanic basins must be infinitely great. The theory that oceanic basins have been permanent has been embraced by many of the ablest geologists, and since sediment has been forming in them uninterruptedly, at however slow a rate, since Eozoic times, its aggregate vertical thickness by now must be colossal. The pressure of water alone upon the rocks forming the bed of the greatest depths of the ocean (say 4000 fathoms) would equal 6195½ tons upon a square yard, and this pressure exists on a bottom which is at or near the freezing-point. The effect would be as if on land the pressure of the first 7000 or 8000 feet of rock generated no heat whatever, or rather as if the heat were intercepted by an icy layer, which also might conduct it away, with the result that the molten layer would rest under a greater weight under the ocean, where the rocks have been observed to be denser, than it does under the land. This extra weight, even if small, would tend to render the greatest depths of the ocean permanent, but lines of current, where sedimentation was less rapid, would present lines of relatively less resistance, which, becoming more and more elevated, would in time form submarine ridges or banks or dry land, until the extreme tension might possibly become relieved by the eruption of volcanic matter. The lines of absolutely least resistance would, however, most frequently perhaps coincide with sea-margins, because these would often be the nearest lines free from the pressure of accumulating sediment. While, therefore, actual shore-lines may be depressed by local sedimentation, there may be inland a far more important tendency to elevation. The recent mountain chains, whether volcanic or otherwise, follow at a distance the contours of coasts, and it is likely that such apparent exceptions as the Alps, Urals, and Himalayas were in proximity to coast-lines at the time of their formation.

To this extent, I believe, the permanence of ocean basins can be maintained, but the past and present distribution of both plants and pulmonate mollusca, which alone in terrestrial life seem to have any antiquity as species, appear to be wholly against any further extension of it.

That such considerations, theoretical as they seem, may have a practical value to geologists, a recent journey to Iceland abundantly proved. There is not only there, but in the Faroes, evidence of a period of quiescence between two great basaltic formations, during which plants grew and lignite was formed. It even appears that this quiescent period extended synchronously from Ireland to Greenland. During this time the Lower Eocene flora, splendidly represented at Reading, seems to have migrated through increasing temperature as far north as Greenland, for the Reading plants are almost wholly those which were thought to be characteristic of northern Tertiary floras and distinctive of Miocene time. A connection between Europe and America in these high latitudes has also been inferred on many grounds to have existed at about the same period. Does it not seem as if the elevation of land (which permitted these floras thus to migrate, and which probably raised the Eocene temperature by excluding the Arctic Ocean from the Atlantic) caused these stupendous eruptions of basalt to cease—for elevation on such a scale must mean relief from tension—and that its submergence during the Miocene led to, or was caused by, the renewal of the basaltic flow? The horizon in Iceland is marked by only very scattered sedimentary tuffs and lignites, and is far less marked than in the Faroes, but in the region of Akreyri it can be traced even from a distance by the highly-laminated beds of light-coloured trachyte, which seem to have ushered in the new volcanic activity. It would be impossible on internal evidence to assign most of them to any definite age, and it is only perhaps on broad considerations such as these that their geological position may hereafter be fixed, though their immense antiquity may be inferred by the denudation which has furrowed, since their deposition, nearly the whole surface of the island into deep troughs and high ridges, out of what were formerly continuous tabular sheets of basalt.

It would be interesting to ascertain whether the great basaltic outpours of Oregon and the Deccan preceded or accompanied any marked changes of level in adjoining areas.

The question has been asked as an objection to this theory, What possible mechanical properties can we attribute to the upper strata of the earth which will permit them to sustain the whole of the Himalayan plateau and North and South America above the sea-level, and yet will cause a continuous subsidence in an estuary in which sediment is being deposited? Such subsidence, it is maintained, could only occur with a substratum somewhere of viscous matter, and if such viscous matter exists, why does it not flow under the stresses due to the weight of continents and mountains?

It is difficult to meet this objection except by appealing to the facts. It is apparent that continents, and especially mountain masses, have been upheaved from below through a pressure which the earth's crust is not rigid enough to resist, and that so long as this pressure is sustained they must remain at least stationary. There is no proof anywhere that the pressure that caused the elevation is now removed, but there are frequently indications, such as earthquakes and landslips in mountain chains, that it exists and is even on the increase. On the other hand, there is no evidence of any kind to show that some, especially of the older mountain chains, are not sinking, though subsidence in such cases would be very difficult to detect. Besides this it is conceivable that when the force which has squeezed the crust into folds has ceased to be exerted it is not flexible enough to regain its original horizontal position, but will remain in folds, and as there is no increased thickness, and consequently no addition of weight, but on the contrary a continual loss from denudation, there is no reason why they should not retain their position upon the hypothesis of a continuous molten layer subjected to greatest pressure at its lowest levels. Dr. Fisher even assumes that the mere removal of weight from them by denudation, and its accumulation on their flanks, would suffice to cause a continuous upheaval. The deflection of the plumbline has shown that the density of the crust beneath mountains must be less than that below the plains, and the relatively slow rate at which heat increases in boring through them shows also that the pressure there cannot be so great. Though strata are compressed into a smaller area through the folding, it is doubtful whether the aggregate pressure on the liquid layer in such regions is at all increased, while in elevated plains it obviously cannot be so, as there is in that case no direct increase of weight. It thus seems as if it were as necessary that the crust of the earth should yield to increasing pressure as that the sea should roughen under the wind, and the apparently arbitrary upheavals and depressions are brought under a definite law. The greatest depths of the ocean would ever deepen and its superficial area tend to diminish, while that of the dry land would increase, and its mountain chains reach higher elevations. The theory appears in harmony with the truths of geology and of astronomy, for the records of Palæozoic times show neither evidence of great depth of sea nor mountainous elevation on land, the organic remains pointing to a little varied surface. The highest mountains are geologically the most recent, and evidence of deep seas increase towards the Cretaceous period, while our satellite, whose evolution may have progressed more quickly than ours, has relatively far greater, more numerous, and more abrupt elevations than the earth.

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[1] Geikie, *l.c.*, p. 287, 1° F. for every 50 feet.

[2] "Physics of the Earth's Crust."

Somewhat similar conclusions to these have been arrived at in the "Physics of the Earth's Crust," by Dr. Fisher. Without presuming to compare the present superficial treatment of the subject with that great and philosophical work, some important differences will be observed between the views there expressed and these, as well as some entirely new observations and extensions of the theory. The views advocated are still so far from being generally accepted by geologists that their publication in NATURE will doubtless put many in possession of facts and inferences which are in a general way only accessible to those who have leisure to gather them from less popular publications. J. STARKIE GARDNER

THE ISCHIA EARTHQUAKE

ONE of the most disastrous earthquakes on record occurred in the little Island of Ischia, in the Bay of Naples, on the evening of July 28. It was only in March 1881 that a similar catastrophe occurred at the same place. The island is a favourite summer resort of Romans and Neapolitans, and Casamicciola, where the destruction was greatest, was crowded with strangers. The full extent of the loss of life has not yet been ascertained; but up to the present it is estimated that at least 4000 have been killed, and very large numbers wounded. The earthquake occurred at half-past nine, when strangers and natives were enjoying themselves in various ways under a cloudless sky with not a breath of air stirring. Not the slightest warning seems to have preceded what occurred; in the space of fifteen seconds Casamicciola was a heap of ruins, while a similar fate overtook the smaller towns of Forio, Laco Armino, and Fontana Serrata. At present we can only record the facts of the case; when further details are to hand it may be possible to throw some light on the real cause of the catastrophe. Besides the first shock, which lasted fifteen seconds, other two were noticed immediately after. Prof. Palmieri is stated to have expressed the opinion that the catastrophe was caused by a sinking in of the level, and not by an earthquake. On the 31st there was another slight shock; while Vesuvius is in a state of active eruption. A Rhenish journal states that on Saturday night, about the time when the Ischia earthquake occurred, a tremendous motion of the earth was distinctly felt at Wiesbaden. On the morning of the 31st also, it may be noted here, a shock of earthquake was felt in Oporto, lasting two seconds, with direction east and west; it naturally caused great consternation. Two shocks are reported to have occurred on the same day at Gilroy, California. With regard to the volcanic Monte Epomeo in the Island of Ischia, we may say that its last recorded eruption took place in 1302.

We are glad to learn that Dr. Dohrn, director of the Naples Zoological Station, who was in Ischia at the time, escaped unhurt.

THE AGRAM EARTHQUAKE[1]

IN connection with the Ischia Earthquake, the official report of the Agram Earthquake of three years ago may not be without interest. The detailed report by Herr Hantken von Prudnik contains all the information which he had been able to collect regarding the severe earthquake with which the district surrounding the town of Agram in Croatia was visited on November 9, 1880. Herr von Prudnik gives not only an exhaustive narrative of his own observations of the effects of the earthquake, made a few days after its occurrence, but also some account of careful observations made by inhabitants of the district where the earthquake actually took place; and his memoir is full of most interesting matter to seismologists. The district is situated in an area within which earthquakes are of very frequent occurrence, for Herr von Prudnik gives a long list with descriptive notes and dates, beginning with March 26, 1502, and coming down to Nov. 9, 1880, but most of them within the present century, of earthquake-shocks, some of which seem to have been severe, which have been felt in the locality. A few self-registering seismographs erected in suitable places in the district would yield, we think, much valuable information and would detect many of the smaller motions, partaking rather of the nature of tremors, which are no doubt frequent, but which, although of great seismological importance, remain unnoticed where such appliances are not in use.

With regard to the earthquake of November 9 itself, the shock seems to have been very severe, causing as it did, besides loss of life, a vast amount of damage to public and private buildings, especially churches. The details of the damage done, given by Herr von Prudnik, are very interesting, and illustrate very clearly the conclusions which have already been arrived at by seismologists as to the effects of the conformation of the ground in the neighbourhood of a building, and of the structure of the building itself, in diminishing or in aggravating the

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[1] "Das Erdbeben von Agram in Jahre 1880." Bericht an das k. ung. Ministerium für Ackerbau, Industrie, und Handel, eingereicht von Max Hantken von Prudnik, gewesenem Director der k. ung. geologischen Anstalt. (London: Trübner and Co., 1882.)